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TAXONOMY AND EVOLUTION

A REJOINDER

THE writer has great sympathy with much of what "X" has to say on the above subject in a recent number of the AMERICAN NATURALIST (Vol. XLVIII, 369–382).[1] Needless to say, however, he can not agree with all. True there is much in systematic zoology that is slipshod, but till statistics can be produced to show that the percentage of slipshod work produced by systematic zoologists is higher than in other fields of zoology, the writer of this article has a temporary residence in Missouri. He is of the opinion, also, that as great a percentage of the work of the systematic zoologist will stand the test of time as the work of the anatomist or any other worker in the field of zoology and proposes to remain of that opinion until time, the great leveler, proves to the contrary.

Linnæus is apparently not the only genius that has left the back door open and that has "been followed by a crowd of other workers eager to attain to immortality," as witness the great mass of half-digested literature on genetics, say, that has been crowded into the past ten or a dozen years. It would be a sad state of affairs indeed if systematics as a whole were not improving. That there have been occasional backward steps there is no doubt, but on the whole the progress has been forward. I hardly believe that even the systematists are as big fools as "X" pictures them to be, for I have yet to discover in my rambles a systematist who believed that his work was final. Heaven forbid. The czar in zoological nomenclature may arise and issue his fiat, but there will be later czars who will do away with them. For surely "X" would not have us believe that the day will ever dawn in this world when all things are settled. My shorter catechism is somewhat awry, but surely such a happy state is reserved for the Great Beyond.

Without wishing to disparage the modern workers I wish to say that some of the older workers did write "careful descriptions," as witness the following case which has been called to my attention. One of the early systematic entomologists described a species, on external characters only, in about three lines. Later entomologists were puzzled because the species had characters common to two widely separated genera; and one systematist said it belonged to one genus, and another said to a widely divergent genus, while a third said it was simply another name for a common form. Yet, behold, when the species was rediscovered it was found to belong to a new genus with characters common to the two widely divergent genera. Now, what's the answer, certainly the original description must have been a good one otherwise how could workers nearly a century later recognize the characters?

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[1] The present paper was written in July, 1914, soon after reading the paper by "X." It was laid away but now that it is more than four years old "going on" five, as children say, it seems best to submit it for publication.

Isolated quotations from descriptions of any species look ridiculous (p. 370), but no more so than isolated quotations from the work of ecologist, neurologist or what not. A kindly feeling for my fellow workers in other fields and for the editor of the AMERICAN NATURALIST stays me from quoting at length and verbatim. Fortunately "X" has sufficiently concealed his identity so that I can not quote some of his own discussions until he yawns. Neither is my soul more deeply stirred by contemplating the poor hymenopterist, squinting at his box of dried "bugs" stuck on pins; than it is by the poor hunch-backed short-sighted cytologist (let us say) who, peering through his high power compound microscope, imagines that the world is circumscribed by his field of view and that a cell, or a nucleus, or a chromosome, is all there is to zoology.

"X" seems to deplore the fact of specialization in zoology and at the same time seems to ignore the fact that it is along these lines that the world moves. Why should we not have neurologists, taxonomists, hemipterists, etc., in zoology just as we have masons, carpenters, roofers, painters, tinners, etc. How many railroads would have been built in this world or how much progress would have been made in any other line of human endeavor if every man had to be a jack-of-all-trades? Do we hire a man to build us a house? Most certainly not. We hire a brick mason to lay the foundation, a carpenter to erect the frame, another one to put on the weather boarding, and still another to do the finishing inside; and so on until our house is finished and the whole structure stands only as long as the work of each one of these individual workers will stand. So it seems to me it is in zoology, the systematist lays the foundation upon which the whole structure is raised. And while the whole method of systematic zoology is open to criticism by anatomists, or what not, yet a certain amount of systematic work must be done before the anatomist can develop his work. If we take this position it seems to me that we must grant that the systematist must be far to the forefront, well in advance of the workers in other fields. And certainly this much must be said in his favor that he has turned out enough "new species" in the last few years to keep the rest of the zoologists busy for a year or two.

"X's" whole attitude is that the systematist makes mistakes and that he sticks only to external characters. In regard to the first I would call "X's" attention to the fact that anatomists a little less than 300 years ago believed the arteries carried air, not blood. And it seems to me if we go back about 250 years we find one Robert Hooke describing "little boxes (empty) of cells distinct one from another"; and wasn't it only about half a century ago that the cytologist awakened to the fact that the boxes were not as empty as might seem? Now the question to my mind is this, would we know as much about cytology as we know to-day, if Hooke had not discovered his empty boxes? I think not. And as a necessary corollary would we know as much about the animal world as we now know if systematists had not described new species? I think not. The fundamental basis of systematic work, it seems to me, must always be external characters, though they may be variable and unsatisfactory in many respects. What we all want and what I believe all systematists are striving for though some of their strivings may be misdirected is, among other things, ease of identification which, to my mind, implies reference to external characters. I see a woodpecker sitting in a tree and identify him as a yellow-bellied sapsucker by the fact that he has, among other characters, a white stripe down his wings. Very unscientific, I grant, but highly satisfactory to me if I am collecting not sapsuckers but downy woodpeckers. Also to the sapsucker if the alternate character which enabled me to identify him was the presence of extra small convolutions on his cerebellum.

I make my plea for systematic zoology as systematic zoology, not for its "phylogenetic classification of animals," nor for its bearing on geographical distribution, variation or heredity or anything else. The description of "560 new species of Zonitidæ" may not seem soul-inspiring work to ''X,'' but to the describer it may have been exceedingly so. The description of 560 new species of Zonitidæ makes it possible for some student of variation or of ''phylogenetic classification'' to work on the Zonitidæ in a way that would not have been possible if these 560 ''new species'' had not been described, and no one man would have been able to describe the 560 new species and work their embryology, internal anatomy, neurology, ecology, geographical distribulation, behavior, variation, mendelian relations, etc., and live to tell the tale.

Furthermore, if there is any man that has the aptitude to describe ''560 new species of Zonitidæ'' my benediction is ''let him go to it.'' And while 585 of his ''560 new species'' may prove to be false alarms that have never been turned in, at the same time it does not seem to have occurred to ''X'' that he may be doing much less harm thusly employed than if he were rampant with scissors, scalpels and needles or with killing agents, stains and a microtome trying to discover the true inwardness of the Zonitidæ. I do not want to be misjudged by any one who may think that I am making a plea for slipshod work, but I do want to make a plea for the isolated worker who is plodding away in his own particular field without hope of reward or recompense in this day or generation. Let us be very careful about setting our stamp upon a thing as worth while or not worth while. Mendel, the poor isolated monk, working away with his peas, never dared dream, I venture to say, that his work would revolutionize the biological thought of the twentieth century. Thus ''X'' may have the misfortune to view in a future reincarnation the sad spectacle of the zoologists of say 200 years hence loudly acclaiming the good work of the describer of the 560 new species of Zonitidæ, while at the same time they point with scorn to the work of the anatomist who discovered (?) that the digestive system of the Zonitidæ runs up hill.

The writer has the fortune or misfortune, as pleases your point of view, to be the entomologist of a state experiment station. His principal duties as entomologist are the intensive studies of two widely separated species of extremely injurious insects. This work is carried on under the Adams Fund by grants from the United States Department of Agriculture. Both projects were so outlined as to involve everything about these two insects that could be discovered by the writer; internal and external anatomy, embryology, life history, parasites, etc. Present indications are that it will take an average of about six years to finish (?) each one of these projects. Yet such a seemingly slow rate of progress is made possible only by the fact that some one working somewhere has described these two species and given them names. The one species was described without the describer ever having seen the male! Yet without this inadequate description progress on this problem would have been very greatly delayed. And so it is in every other field that these problems touch. Some one has described somewhere 29 species of parasitic hymenoptera, one of this number preying upon one of the species involved. Yet the describer knew only the adult and that only imperfectly, but his knowledge plus my own sends us one step nearer the complete knowledge of this species which "X" craves. And our knowledge of this species plus some one's knowledge of other related species raises us just one step nearer the truth which should be the goal of all human thought, and all science, zoology not even excepted.

I am interested in the phylogenetic relationship of a group of insects of no great economic importance. Especially am I interested in the genealogical tree of these insects as shown by the groups of characters of one structure. Now such work is made possible because three men in this country have devoted their entire time describing new species and new genera in this group. Without these descriptions many of which might have served as well as the one quoted on page 370, and without the collections of insects which these three men have made it would be impossible for me to make any progress along the line of a genealogical tree, which it is my fond hope will be of some use to the systematists of this group and to zoologists in general.

I have long wondered what could be called trivial characters. A few illustrations of the importance of so-called trivial characters in other fields than systematics may perhaps occur to "X." One of the most important that has come to my notice was that of a cytologist who discovered differences in the chromosomal characters of two different sets of individuals of the "same species" only to discover later that systematists had long distinguished between these two forms on the basis of characters more trivial than whether they were "pink with blue spots" or "blue with pink spots." Again two species of scale insects are separated by the fact that one has the median lobes of the pygidium rounded while the other has the median lobes conical (external characters). Yet one lives on oak trees and has at least four generations annually and the other lives on maple trees and has only a single generation annually. Now if "X" thinks that these facts would have been discovered as easily and as quickly as they have been discovered, if Professor Comstock had not pointed out these "trivial characters" some thirty-odd years ago, he thinks differently than I think. Yet the application of these facts is of vast importance to the horticulturalist and landscape architect or any other artisan who works to beautify our landscapes with trees, or any one who attempts to control these two pests.

I have no doubt that Linnæus was accused of relying on trivial characters for separating some of his genera and species. It would be interesting if history could tell us and it would be still more interesting if we could look into the future, say 100 years, and see what systematists and others will say about the present-day systematists who overlooked such perfectly obvious characters as the extra spines on the hind leg of species "a" as contrasted with "b" and their wonder and amazement that systematists of this our glorious twentieth century should have overlooked such important and obvious characters. So it will be in other fields. The histologist of the future will wonder why we used such crude killing and fixing agents; and will, more than likely, refer to our finest precision microtomes with a shrug much as we refer to the stone hatchets of the men of the Old Stone Age.

I make this somewhat extended plea because it seems to me that "X" has unconsciously done the systematists a great wrong.[2] "X's" attitude may discourage promising young men from entering the field of systematics where their help is greatly needed. Let us therefore lay aside our critical air and our sitting in judgment to decide just what is worth while and what isn't and turn our attention to utilizing the results of other workers in other fields to the greatest extent. The systematist is human like the rest of us, he has his limitations like the rest of us, but he believes, I think justly, that his work is pioneer work of great importance; and, if occasionally he gets beyond the limited range of our embryologist's microscope or our anatomist's scalpels and needles, let us not accuse him of wandering along the River of Doubt or being a lineal descendant of the famous Baron Munchausen. But let us look upon the systematist's work as the foundation for the glorious structure, modern zoology, which completed by his other co-workers will stand four square to the wind for all time to come. We do not need to defend systematics on the basis of "(1) the advertisement theory; (2) the recognition mark theory;" although both are perhaps more important than "X" intimates. But what is vastly more important is the fact that systematics is the basis for all real work in zoology. And the morphologist or anatomist who takes the attitude that systematics is to be entirely avoided[3] or, what is worse, is to be simply laughed at is placing himself in the same class as the man who says that there is no such thing as matter in the world. Sooner or later he is going to bump into the fact that systematics must play its part in his field and that systematics is broader than the question whether the "second joint is longer than the third" or whether a species should be called aabus Smith or beabus Jones.

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[2] That all may see that my plea is entirely unselfish, I will state that I am not a systematist and that I have never described a single "new species." My attitude is simply one of gratitude to the systematists who have helped me with my problems.

Z.

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[3] Just what does "X" think about the anatomist who discussed at great length the anatomy of, let us say, the "American frog (*Rana temporaria*)" because that was the name given the frog in his perfectly good English "Text-book of Zoology"; when the context shows that the frog he was dealing with was the common leopard frog (*Rana pipiens*)?